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Oral History Interviews
Medical Physics Series

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History of Science and Technology Program

WILLIAM G. MYERS:
EARLY HISTORY OF NUCLEAR MEDICINE

An Interview Conducted by Sally Smith Hughes

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WILLIAM G. MYERS

Contents

Acknowledgment iii
Introduction iv
Curriculum Vitae (Myers) vii

Early Interest in Artificial Radioisotopes 1
The Bikini Atom Bomb Tests 2
Clinical Use of Phosphorus-32 4
Radioisotope Distribution by Oak Ridge 5
Cobalt-60 6
The Carbon Isotopes 11
Cobalt-60 (continued) 13
Benedict Cassen's Rectilinear Scanner 18
The Anger Scintillation Camera 19
Iodine Isotopes 20
The Society of Nuclear Medicine 22
The First Commercial Anger Camera 24
Georg Hevesy 28
Neutron Therapy 29
The Society of Nuclear Medicine Emblem 31
Georg Hevesy (continued) 32
The Origin of the Term "Nuclear Medicine" 34
Terminology 35

Appendix: Hevesy Nuclear Medicine Pioneer Lecture, by Henry N. Wagner 38
Index 42
Curriculum Vitae (Hughes) 44

Acknowledgment

This interview with Professor William G. Myers is one of several dealing with the history of nuclear medicine and the development of Crocker and Donner laboratories, within the larger series of oral histories produced by the History of Science and Technology Program.

Besides these interviews, the Program assembles other primary source materials, including the papers and personal memorabilia of scientists and engineers, and the papers of certain organizations with which they were associated. The papers and interviews help to document the development of science and technology not only in the western United States, but also in the nation as a whole.

The work of the Program was made possible initially by the generosity of William R. Hewlett and David Packard. Mrs. Calvin K. Townsend established the Doreen and Calvin K. Townsend fund to provide ongoing support of the Program. The University Endowment Fund, National Science Foundation, and National Endowment for the Humanities have assisted diverse aspects of the Program with a series of grants. Further aid has come from the Marco Francis Hellman Fund, established to document science and technology and their relations to business in California. Other donors have included the Woodheath Foundation, the California Alumni Foundation, and the Watkins-Johnson Company.

James D. Hart
Director
The Bancroft Library

Introduction

In the spring of 1982 Sally Smith Hughes wrote to Dr. Myers, the historian of the Society of Nuclear Medicine and a nuclear medicine pioneer, asking for information about the early history of nuclear medicine. In April 1982 she received a phone call from Dr. Myers saying that he was in Berkeley for a few days and would see her at the Faculty Club the following morning to discuss the reprints he had brought from his own historical collection. Dr. Myers kindly allowed her to tape the unrehearsed discussion, which is transcribed in these pages.

The discussion occurred as they went through the pile of papers in front of them and hence does not follow the chronological or thematic sequence common to most interviews. In editing, several sections that were merely comments on the reprints have been omitted. Jennifer Snodgrass and Marie Herold aided in the final production of the transcript. The index was prepared by Robin Rider.

As mentioned in the discussion, Dr. Myers' initial concern with the cyclotron and artificial radioisotopes began with his M.D. thesis of 1941 on the biological applications of the cyclotron. (A copy of this thesis and many of its references can be found in the library of the Donner Laboratory.) In 1949 at the Ohio State University School of Medicine he started the first continuously taught course in nuclear medicine. In 1958 he became acquainted with Mr. Hal O. Anger of the Donner Laboratory at the University of California, Berkeley, who invented the scintillation camera that now bears his name. Dr. Myers subsequently visited Berkeley many times to follow the development of this and other imaging devices made by Mr. Anger. At Dr. Myers' instigation, Ohio State became the owner of the first commercial version of the Anger scintillation camera in September 1962. Partly because of Dr. Myers' efforts to find and to promote clinical applications for the Anger camera, this instrument and its successors play a central role in present-day nuclear medicine imaging techniques. An interview with Mr. Anger and Dr. Donald Van Dyke in this series provides further information on the development and use of the Anger camera.

This interview is part of a series recorded during 1979–1980 with individuals associated with Donner and Crocker laboratories and, in some cases, the Division of Medical Physics and Biophysics. Donner Laboratory is part of the Biomedical Division of what since 1971 has been called the Lawrence Berkeley Laboratory; Crocker Laboratory no longer exists. The Division of Medical Physics and Biophysics, which in March 1980 became a department, is an academic unit of the University of California, Berkeley.

This medical physics interview series, conducted under the auspices of the History of Science and Technology Program at The Bancroft Library, is part of a project funded by the National Endowment for the Humanities to document the history of the Donner and Crocker laboratories, the Division of Medical Physics and Biophysics, and the careers and research of scientists and physicians associated with these institutions. These interviews, in conjunction with archival holdings at The Bancroft Library and Lawrence Berkeley Laboratory, illustrate the growth of the fields of medical physics and biophysics, in which Berkeley research and academic institutions played an early and significant role. They also complement other interviews and documents in Bancroft Library concerning the development of Lawrence Berkeley Laboratory.

Oral history can frequently provide information on subjects not easily retrieved from published sources. These subjects include family history, social and economic conditions affecting research, interactions with colleagues, peculiarities of institutional organization, and perceptions of a scientific discipline. It should be borne in mind that the interview records what the subject remembers during the interview about what happened at a given place and time; typically, many years have passed since the events occurred. In general, information obtained in response to broader questions is more likely to be accurate than answers concerning specific events, influences, and accomplishments.

Literary rights for this interview are vested in the Director of The Bancroft Library. Any quotation for publication of the material included herein requires the advance written approval of the Director. A request to see the transcript constitutes an agreement to abide by this restriction.

Background materials pertaining to this and other interviews in the History of Science and Technology Program series, together with tape recordings of interview sessions, are available for research use in the Heller Reading Room.

Sally Smith Hughes
Robin E. Rider
History of Science and
  Technology Program
The Bancroft Library

CURRICULUM VITAE—William G. Myers

1908	Toledo, Ohio, born August 7 father: Leo J. Myers mother: Anna C. Johnson Myers
1933	Ohio State University, B.A.
1937	Ohio State University, M.Sc.
1939	Ohio State University, Ph.D. (physical chemistry)
1941	Ohio State University, M.D.
1941–42	Medical Intern, Ohio State University Hospital
1942–45	Research Associate, Ohio State University Research Foundation
1945–49	Julius F. Stone Fellow in Medical Biophysics
1946	Monitor, Operation Crossroads, Bikini
1949–53	Associate Professor, Ohio State University
1952–64	Consultant, U.S. Naval Medical School
1953–79	Professor, Ohio State University
1970–	Visiting Professor, University of California, Berkeley
1973–	Historian, Society of Nuclear Medicine
1979–	Professor Emeritus, Ohio State University
1979–	Visiting Professor of Biophysics, Cornell University Graduate School of Medical Sciences
1979–	Visiting Investigator, Memorial Sloan-Kettering Cancer Center, New York City

Date of interview: 17 April 1982

EARLY INTEREST IN ARTIFICIAL RADIOISOTOPES

Myers: My so-called M.D. thesis really was a sort of a glorified term paper on the application of the cyclotron up to that time. It was accepted by Dr. Charles Austin Doan in February of 1941. I date my interest in the subject, actually, from Dr. John Lawrence's Sigma Xi lecture, at Ohio State University in November 1940, although I had heard his brother, Ernest Lawrence, give a Sigma Xi lecture at Ohio State in early 1935. This was one of Ernest Lawrence's longer publications, incidentally. It's published in *The Ohio Journal of Science*.

Hughes: I haven't seen that.

Myers: Right there in 1935. Ernest Lawrence really didn't write many papers.

Hughes: No, that's true.

Myers: But this [pointing to paper] is one of the longest ones. This was along about March or April of 1935 that he mentioned that the discovery of man- and woman-made radioactivity had been made in Paris only fourteen months previously, and that they already had created here [at Berkeley], with the 27-inch cyclotron, a large number of radioisotopes, many of which are still used in medicine.

Hughes: How were you determining dosage with the P-32?

Myers: Well, we based it on the "Berkeley" unit. They sent us a sample of P-32, and we simply evaporated it on a watch glass and put it [at] the same geometry as we had with the electroscope. There was a fixed geometry with the electroscope. We measured the rate of discharge with that instrument. If we had a stronger sample, it would discharge more rapidly. If we had a weaker sample, it would discharge more slowly and so on.

THE BIKINI ATOM BOMB TESTS

Myers: Actually, I used that method of measuring P-32 when we started getting it from Oak Ridge. We got our first shipment from Oak Ridge in September of 1946. I'd just come back from the Bikini atomic bomb tests. Dr. John Lawrence, Dr. Hardin Jones, and Dr. Joseph Hamilton were all there from Berkeley.

Hughes: And Dr. [Kenneth G.] Scott, too.

Myers: Yes. I got to know all of them at Bikini. Dr. John and I were the only two scientists aboard a gunboat that was monitoring radiation for this operation when the underwater A-bomb shot took a battleship and stood it right on end. That was in 1946, on July 25th.

Hughes: Were you there to observe the medical consequences?

Myers: We were there as radiation monitors.

The story on that—this is partly surmise—to be in charge of the radiation monitoring aspects of the atom bomb tests was Dr. Stafford Warren, who later then became the first dean of the medical school at UCLA. I think he was a colonel, or something like that, in the

army. Right after the Hiroshima and Nagasaki events, the scientists left the Manhattan Project. All the first-rate scientists quickly went back to their university posts. The war being over with, and the people being discharged and anxious to get out of the armed services, they had very few people that they could use at the Bikini A-bomb tests.

What happened was Dr. Stafford Warren knew that physicians could be trained quickly, and in problems of radiation considerations they would be conservative, and so on, in case things got away from us out there. So what he did was to call Dr. Doan, the same man, and said, "Would you come out?" By this time Dr. Doan was the dean of the Ohio State University Medical School. Apparently, Dr. Warren called all the deans of the medical schools of the United States, by telephone, and invited them out.

Most of them couldn't come. So Dr. Doan told him about me, that I'd been working in this field for some years, and so on. We also had Dr. [Marion L.] Pool, from our physics department, who was then in charge of the cyclotron at Ohio State. So we all showed up out there. That's how Dr. John Lawrence, Dr. Hardin Jones, Dr. Joseph Hamilton [got to Bikini]. These are the *real* giants in this field, after a man like [Georg] Hevesy, of course.

We used our cyclotron at OSU not only for making P-32, but then we started using it to make strontium-89, following the leadership here at Donner Laboratory. I've forgotten that man's name. He was a Belgian, and I think he was killed in the war.

Hughes: Pecher. Charles Pecher.

Myers: Yes. Anyway, it was a good idea that didn't work. The problem was that the strontium-89 has a very hard beta particle, but it's long-lived—it's something like 55 days long—and it goes to bone like calcium does, so that it irradiates the bone marrow. Sure, you could irradiate the

osteosarcoma, but the patients would die with not having any blood. So, we abandoned that.

There were a lot of foolish things carried out in the early days. You know, by hindsight. Hindsight is always 20/20 or better.

THE CLINICAL USE OF PHOSPHORUS-32

Hughes: What about the P-32? Can you remember when you stopped using that?

Myers: We're still using it.

Hughes: But not for leukemia any more.

Myers: No, it's for polycythemia.

Dr. Doan kindly included me as one of the coauthors of a paper in 1947, first published in the *Journal of Laboratory and Clinical Medicine*, on our first hundred cases, in which P-32 was tried for all sorts of things. But the thing that emerged as the most successful application of it was for polycythemia vera. That was started here by Dr. John Lawrence, of course.

Dr. John Lawrence is the father of true radiopharmaceuticals. Now, I know he doesn't like this term. I don't either when it's being used for diagnostic applications, but it's appropriate when you're trying to produce a biological effect by "internal" radiation. If you read between the lines in most of Dr. John Lawrence's publications, he was thinking primarily of using radioisotopes to treat the patient. So was his brother Ernest interested in treatment, rather than in the diagnostic applications, although there was a lot of that, too.

Anyhow, we had our first hundred cases. By that time this included the first few shipments we'd gotten from Oak Ridge. I'll never forget that first shipment I got in. I assayed it by this quartz-fiber electroscope method. We had been lucky when we would make ten or twelve millicuries by running the cyclotron all day long every Monday. The first shipment I got from Oak Ridge had 350 millicuries in it! [laughter] We didn't know what to do with so much. And that was the minimum [order]. You took 350 and that was it.

RADIOISOTOPE DISTRIBUTION BY OAK RIDGE

Myers: The first shipment from Oak Ridge of radioisotopes for civilian applications was on August 1 of 1946. So we were there right near the out[set]. The published information about the availability of radioisotopes came out to us at Bikini while I was still there. I came back in early August and immediately filled out all this folderol and so on, and we got our first shipment in September.

Hughes: What were the conditions at that time, do you remember, for receiving shipments?

Myers: They were anxious to have people use them. Dr. Paul C. Aebersold was actually in charge of running the cyclotron here, not only the old 37-inch, but also the 60-inch cyclotron. Then he was taken from here to work at Los Alamos during the war. His son (that I'll be seeing tomorrow), who took his Ph.D. degree here at Donner Lab just a couple of years ago—Paul M. Aebersold—he's working here now in the field of genetics.

Hughes: I didn't know that. Anything to do with radiation?

Myers: I don't know. I haven't seen him for a couple of years. (He's my godson. We became very close friends with the Aebersolds.)

Paul C. Aebersold went from Los Alamos to Oak Ridge and he was put in charge of setting up and being the first director of the isotopes division of the Atomic Energy Commission in Oak Ridge.

COBALT-60

Myers: I got the idea at Bikini to study cobalt-60 as an advantageous successor to radium. When I came back I wrote it out to Paul C. Aebersold that I'd like to use radioactive cobalt-60 for cancer treatment, because of the hard gamma rays, and also gold-198. These all came to pass. He was very enthusiastic about it, and helped me in every way possible. As you know, now cobalt-60 is the most commonly used way of treating cancer with radiation.

Hughes: Yes. When was the first application?

Myers: For the first application Mrs. Myers and I drove to Oak Ridge from Columbus—it's a distance of about 400 miles—in May of 1948. I'd gotten a few shipments previously, but they wouldn't send very much. The gamma rays from cobalt-60 are very energetic—1.25 MeV.

We stayed with the Aebersolds over the weekend. We drove our Packard—that will date it for you. I had them build a wooden platform in the trunk of this Packard to distribute the weight. We had a big shield to provide 4.5 inches of lead between us in the front seat and the cobalt-60. We had monitoring instruments by that time, and the people at Oak Ridge monitored it also.

On Monday morning I filled up the tank with gasoline, and filled up two extra 5-gallon cans so we wouldn't need to stop at a filling station on our way back to Columbus. I drove the Packard directly out to the old graphite reactor. They took it from the reactor and put it in the lead shield in the trunk of our car, and monitored the radiation levels. We drove it all the way back to Columbus that day.

The first use of it was [by] our professor of gynecology and obstetrics, and a colleague of mine in radiology. They put it in the form of a plaque to treat a cancer of the vagina in a woman.

During the summer of '47 I had made needles. Dr. Aebersold had suggested a source of wire that was used for filaments which was made up, essentially, of equal parts of cobalt and nickel. The machinists wouldn't play with this stuff. I had to make my own needles. I bored eyes and ground points on them, and so forth. I had needles 1 millimeter in diameter and 2 or 3 centimeters long. These were incorporated into the plastic plaque. The first treatment, then, with cobalt-60 was in August of 1948 at the old Ohio State University Hospital.

In that same year I published three or four papers about cobalt-60. In them I proposed the possibility of making a device where you can have hundreds of curies—I was too low, I should have said thousands of curies—of cobalt-60. Of course, these are now used world-wide. Thousands of them are in use.

Hughes: What was the reception of the medical profession?

Myers: Well, I have learned that the longer-lived a radioisotope is that you advocate—what I call a convenience coefficient—the more likely it is to succeed.

I remember when Hal Anger and I introduced iodine-123, just 20 years ago, with its 13-hour half life, lots of people said it won't fly; it's ridiculous; nonsense.

A lot of people were sceptical of the cobalt-60, also, because up to that time the teletherapy devices had radium in them and they never had more than ten grams. Well, by simple calculation, we could show that you could have the equivalent of kilograms of radium in a small point source.

In 1950, the Aebersolds and Mrs. Myers and I and another couple went over on the Ile de France to Europe. We participated in scientific meetings in Paris, London, Copenhagen, Stockholm. We visited ten different countries together. Because of his position, the red carpet was thick wherever we went. It was really a fabulous trip.

Anyway, during that first year or two I got in many other radioisotopes. I got in antimony-124, mercury-203, selenium-75, sulfur-35. And we began to get some iodine-131 in from the Oak Ridge reactor.

We went over on the Ile de France and came back on the Liberté together. We went over in June and came back in September. We were together constantly.

Hughes: Were you pushing radionuclides?

Myers: That was our principal reason, yes. Well, we went to visit many of the facilities. We saw most of the cyclotrons in Europe at that time. At Harwell they shut down the reactor so we could crawl around on top of it.

Hughes: The British were producing enough?

Myers: Oh yes. They were starting to use it.

We saw many applications of it. For ourselves, we had an exhibit on cobalt-60 at the International Congress of

Radiology in London in late July or early August of 1950.

I remember one time I had an exhibit, and Joe Hamilton had an exhibit right beside me. Whenever the pub would open, we would go across the street and have a high beer for [B.V.A.] Low-Beer—that sort of thing. Bertram Low-Beer was also a real pioneer and a very dear friend. He wrote the first book in the English language on the uses of radioisotopes in medicine. That was in 1950.

Bert and I liked snails. Always, wherever we went, whoever found where you could get snails first, the other one would have to go with him and buy the snails. And we always had a high beer for Low-Beer! Ann, his wife, lives in San Francisco. She really knows the early history. What was her [maiden] name?

Hughes: Treadwell, I think.

Myers: Yes. It was her paper that suggested the use of the strontium-89 for treating osteogenic sarcoma. I met her here about eight or ten years ago. It's the only time.

Hughes: I actually talked to her. She's remarried now. I was hoping that some of Dr. Low-Beer's manuscripts and lab notes, and that kind of thing, still existed. She didn't know anything. She thought probably they'd been destroyed.

Myers: That's too bad.

I talked to a lady here who worked with Joe Hamilton closely, on astatine.

Hughes: Pat Durbin.

Myers: When I was here five years ago I talked to her. She said the same thing, that these were largely destroyed. That's

a pity.

Anyway, my Ph.D. degree was in physical chemistry in 1939. I was interested in [radioisotopes] from the standpoint of treatment, with the cobalt-60 first and then with the gold-198, which was used quite widely throughout the world and is still the most commonly used way of treating cancer with radiation in England at the present time, so I've learned.

One thing led to another. At first I was interested in fairly long-lived radioisotopes. As a matter of fact, I became the historian of the Society of Nuclear Medicine nine years ago. For instance, we're having a big international meeting—the Third World Federation of Nuclear Medicine—in Paris this August. The secretary of the congress called me from Paris about a month ago and wants me to give, at the closing ceremony, an historical overview. I did the same at the second congress, in the final closing ceremony.

Hughes: Was that written up?

Myers: No. I don't have time. I'm getting too old. Right now, since I retired at Ohio State three years ago, I spend about a third of my time at Memorial Sloan-Kettering Cancer Center in New York City. I just got back, ten days ago, from a second trip. I usually go for two weeks at a time, six or eight times a year, and get data.

We're going to have two exhibits at the Paris meeting. One of them is an old idea that originated here in Berkeley. The use of just plain carbon dioxide labeled with carbon-11.

THE CARBON ISOTOPES

Myers: The first use of carbon-11 was here in Berkeley in the form of carbon-11 dioxide, by [Samuel] Ruben, [W. Z.] Hassid and [Martin] Kamen in 1939. They had about three or four, possibly five, publications in the *Journal of the American Chemical Society*. They were interested in it from the standpoint of following the chemistry of carbon, particularly in relation to photosynthesis. So their work really antedated the work of [Melvin] Calvin much later.

I've heard Martin Kamen—he lives down in San Diego—tell this 25 years ago. "As a biochemist I just felt that nature couldn't be so cruel as to provide us with a radioisotope of carbon with a half-life no longer than 20 minutes." So he and Ruben, in 1940, talked to Ernest Lawrence about it. He was enthusiastic—he was enthusiastic about everything—that they should try to see whether they couldn't make a longer-lived radioisotope of carbon with the cyclotron. With the 60-inch machine they bombarded carbon—I think it was enriched in carbon-13. Every time [the cyclotron] wasn't used for something else they'd put this target on and bombard it. They discovered carbon-14 in 1940.

I have an old isotope chart at home, [made] about a year later. It gives on there "carbon-14, half-life about 3 years." [laughter] I like to illustrate this to my students. I taught a basic core course in nuclear medicine for 30 consecutive years at Ohio State. I would point out that even physical scientists can miss it by 2000-fold!

Hughes: Maybe you know this, but Ernest Lawrence made the announcement of the discovery of carbon-14 at the very end of his Nobel Prize lecture. Did you know that?

Myers: I didn't know that.

Hughes: Because of war conditions he didn't go to Stockholm.

Myers: Yes. I know that.

Hughes: There was a ceremony right here on campus.

Myers: Anyway, in '58 we got interested in an idea I had, chromium-51 for gamma-ray sources for radiation therapy. It never caught on, for the simple reason we never found a good way to make point sources of pure chromium. I won't bore you with all the detail, but that was a technical reason. The idea was good. The gamma rays from chromium-51 are perfect—323 keV, electron capture. No beta particles at all. So you could just use bare sources. There were one or two publications in addition to our own on that, but it didn't catch on. Twenty-eight day half-life.

Back in the late '40s we made about 300 or 400 radioactive compounds. Potentially they were to be labeled with I-131. The older literature on cancer shows that dyes tend to accumulate at the growing edges of tumors. Many dyes do this. I was going to run a "Paul Ehrlich" experiment with "twinkling" atoms, as I like to call them. So what I did was to synthesize 2,6-diidosulfanilate labeled with sulfur-35, simply because it had a convenient, 87-day half-life. I was looking for big changes so I didn't have to have... The radiation for sulfur-35 is the same as that for carbon-14, essentially, so our assay procedures could be very crude, and they were. But I was looking for big differences. Really yes or no.

The reason I had the iodine on there was that then we could shift any time we wanted to to a gamma-ray emitter. At that time, of course, we were thinking of I-131. That was before I-123 was discovered; I-125 was discovered in 1946 at a psychiatric institute in New York

City. It's the most commonly used radioisotope in all of medicine, chiefly because of Rosalyn Yalow's RIA (Radio Immune Assay) methodology.

Anyway, we made these. We tested it in cancers in mice, transplantable tumors and so on. That work went on for three or four years. Then I abandoned it finally. I didn't want to clutter up the literature with a lot of negative stuff that wasn't very exciting. We did find a few compounds that localized in or near the tumor. But I came to the conclusion it wasn't in the tumor—a tentative conclusion—but in the macrophages that were associated with the tumor, simply because the dyes were colloidal particles.

Hughes: They were picking them up.

Myers: I never did bother to write it up. I perhaps should have. I had a couple of little things on it, but that was all.

Then the cobalt-60 got going in a big way, and that took a lot of time. And the gold-198 did also.

Hughes: Was this all with AEC support? Was that where your funding was coming from?

Myers: No. This man that gave the cyclotron to Ohio State, his son, when his father became ninety years of age, established a fellowship—very handsome in those days—and money to support a fellow. I was it, I think, for four years.

COBALT-60 (continued)

Myers: As a matter of fact, the cobalt-60—when we were working on that—Mr. [David] Lilienthal, the first AEC chairman, went in to see the President, Mr. Truman at that time, along about April of 1948. It was on a Saturday. I well

remember I was over in my lab working. The telephone started ringing right after lunch. I was never able to get it real straight; much of this is surmise. But apparently the reporters were there when Mr. Lilienthal came out of the President's office. Apparently they said, "What can you tell us? What was the conversation about?"

Of course, undoubtedly it was about A-bombs. He just said, "No comment. No comment. No comment." Then they said, "Well, isn't there something good coming out of all this effort?" He said, "Yes. There's some work going on at some university on a new way of treating cancer with radioactive cobalt." But he didn't know where. They wanted to know where right away. He referred them to AEC public relations, or whatever it was. That was when my phone started ringing. AP and UP and IP and all the other "P"s and the local press, *Life* and *Time* and so on.

I got no rest for a long time. I regarded it as a privileged communication, my application of cobalt-60. That made me so very angry that I never asked AEC for a dime, ever. I regard them as being a bunch of self-serving busybodies, in spite of the fact that Dr. Aebersold and we were very close friends.

Hughes: Was that because they were under such pressure to show the beneficial uses of the atom?

Myers: Of course. I don't feel now like I did at the time. The point of it was, my colleague that I was going to work with in radiology was actually at Eniwetok on successive A-bomb tests. All of this broke while he was away. So I was told by people that knew about working with the press that they'll get a story, regardless, and that I had better cooperate with them so they'll get it somewhat straight. So, that's the way we did it. Our own public relations man came over on Monday. We talked it all over, what we had done and so on up to that time, and the proposals. He

prepared a press release and called in the local press. It went off very well. But even so, we had a whole bunch of damn foolishness like, "Cancer Cure Discovered at Ohio State!" That sort of stuff.

So, when my colleague came back from Eniwetok, he would hardly speak to me. It just created havoc. I'd get all the letters like, "Doctor, you're on the wrong track. If you want to cure cancer, you eat a certain brand of peas, planted in the moonlight at a certain phase," and so on. I was a bit ticked off. I was getting all these crank letters. But the sad parts are... I remember one. I lost this letter, unfortunately. She said, "I'm a maiden lady. We only have men doctors in town so I don't want to go to them, but I think I have a cancer in my rectam [sic]—r-e-c-t-a-m, I remember that—and would you please send me some of your cobalt-60 so I can put it in my rectam and cure my cancer?" That's the kind of things we got.

One day an ambulance pulled up with South Carolina license plates on it, over at the radiology department, and they said, "We read about your new cure for cancer. Papa, here, has cancer. We sold the farm so that we could hire the ambulance to bring him up here. Will you cure his cancer?" They sold the farm just to haul him up!

So we had a lot of foolishness like that. It was heartbreaking, to be sure, but...

Hughes: You had to turn them away at that point?

Myers: Well, there was nothing we could do! We didn't have a cobalt-60 teletherapy unit at that time.

I got laughed at on cobalt-60 too! You asked about that a little while ago. I became known as "Wild Cobalt Bill" and "Cobalt-60 Bill," both in this country and in Europe. A lot of the older radiation therapists laughed at this idea. I'd proposed in writing, and it was published, as

much as a few hundred curies. I should have said several thousand curies, because that's what the machines we have at Memorial right now [have]—a cobalt-60 teletherapy unit now is loaded with many thousands of curies.

Even in Europe, I remember in 1953 at the International Congress of Radiology in Copenhagen, people would laugh at me, and point to "Wild Cobalt Bill." But, I've learned that the *last* laugh is the only laugh that *really* counts!

Then we became more interested in diagnostic applications. Well, another time the AEC, in all of its omniscience, was going to have a high-level conference in Washington in 1957, of the governors of the various states on how to regulate atomic energy within the states. Frank Lousche of Ohio couldn't go for some reason, so I was asked to be his representative. I think I was the only non-governor at this very high-level conference. I kept insisting that the practice of medicine is regulated by the states, in accordance with the 10th amendment to the Constitution, and that therefore the AEC should keep its cotton-picking fingers out of interfering with the practice of medicine. I became persona non grata with the AEC in a big way in a big hurry!

Anyway, after the meeting I went to see the head regulator. He was a lawyer and said, "Well, Professor Myers, if you think I'm going to administer the Atomic Energy Act in accordance with the wishes of individuals, you've got another thing coming."

I said, "Well, if you think you're going to do it on any other than an individual basis, you are creating a disservice to applications of atomic energy in biomedicine." I was so incensed that when I came back—I knew about iodine-131, of course—I thought, iodine has more radioisotopes than any other physiological element. At that time the chart showed something like two dozen. So I thought, "Well, there ought to be something here, because

he can only control radioisotopes that are made incidental to nuclear fission. There ought to be something there that he can't control."

So, my eyes fell on iodine-125 and iodine-123. I made the calculations based on formulations used in my teaching. It turned out that they looked very good. So, I had a young student from Hengelo, Holland, who had been working for me [and] who was an electronics whiz. He'd been working for me for a couple of years. He wanted to take a Master of Science degree with me in biophysics, so one of the professors in physics and I supervised his work. Later he took his Ph.D. degree. His thesis in 1959 was on iodine-125. We presented it the following year.

Now, this has a 60-day half-life and beautiful properties for many applications. Actually, I was quite concerned when I discovered on theoretical grounds it would be possible to make it in a reactor, because then this damn lawyer in Washington could get his hands on it and regulate it. But he couldn't do that with I-123, fortunately.

That was the reason Hal Anger and I teamed up on I-123 in 1961. At that time the crystal in Hal's camera was a little thing, 8 inches in diameter and a quarter-inch thick, up in room 364 in Donner Lab. I told Hal at the SNM meeting in Pittsburgh in June 1961 that he should switch to another isotope of iodine than I-131.

He said, "Which one?"

I said, "Well, I-123 has beautiful properties to work with your camera."

So he got hold of Dr. John Lawrence, and I was appointed to come out here and work during the fall quarter of 1961 on iodine-123.

Hughes: And this tie-in had come just from the meeting where you'd been talking to Anger? That's how you became

interested in the camera?

BENEDICT CASSEN'S RECTILINEAR SCANNER

Myers: Well, no. First of all, the rectilinear scanner was developed by [Benedict] Cassen and colleagues in 1951. It's only 31 years old, the "inside-out" imager. I was able to purchase the first directionalized scintillation detector in April of 1951.

We had a man who had just taken his Ph.D. degree in physics and was teaching in the Department of Electrical Engineering. His name was George E. Mueller. I knew him fairly well, and he said during lunch one day early in 1952, "Why don't we start a new area of interdigitation between electrical engineering and medicine, and call it medical electronics?" or something like that. So that came about. By this time Ben Cassen's paper on his rectilinear scanner had been published in August 1951, and I'd seen the instrument. I said, "I need one of those, but I don't want the mechanical limitation it has."

He said, "Oh, I can get around that with no problems at all. We can do the writing electrically. There'll be no mechanical inertia." So, a year later he built it, and we had one of the first rectilinear scanners in the world, after Ben Cassen's.

This man was then an associate professor of electrical engineering at Ohio State, and we didn't recognize his great administrative ability. He was a very quiet person, mild and so on. Anyway, to make a long story short, he later became the engineer in charge of putting Neil Armstrong on the moon. [laughter]

Hughes: A really capable guy.

Myers: Yes. That testifies as to his administrative capabilities.

THE ANGER SCINTILLATION CAMERA

Myers: I remember when Hal had his first scintillation camera in Los Angeles in June 1958. George Mueller came over, and I showed Hal's camera to him. George agreed that Hal's method of imaging was the way to go.

That meeting was funny. Picker Corporation went into the nuclear medicine field in a big way at that meeting in Los Angeles. I think it was our sixth annual meeting. They had several shapely girls as demonstrators who were dressed with almost nothing on, bouncing on, off, flopping around on the rectilinear scanners. It was air-conditioned and the girls had goose bumps about that high [gesture]. Poor old Hal! They didn't distinguish between the commercial and the scientific exhibits, and there he was, standing back there all by himself. I don't think anybody went back there, excepting [me]. I'd heard of Hal's camera before the meeting. George Mueller came to see it and said, "Oh, yes. This will succeed. This will take care of all the rectilinear scanning. This will be the winner."

The next week [Hal] came up here to San Francisco, at the AMA convention. Again, I was about the only "customer" he had all week. I have beautiful pictures I made of him with his first scintillation camera, in Kodachrome, in June 1958. That's in one of the reprints. We became fast friends. I came out here frequently and gave various talks and so on.

IODINE ISOTOPES

Myers: I told you already that in 1957 I had made these calculations about iodine-123. Iodine-123 was discovered here by Isadore Perlman with the 60-inch medical cyclotron in 1949. At that time it had only one gamma ray—159 kilovolts—which was perfect for Hal's quarter-inch thick crystal. It's now known to have a dozen more gamma rays, all of them harder but not very abundant.

There's going to be a big meeting in Washington on the third, fourth, and fifth of May. It's going to be mostly about I-123. This is a big international conference on short-lived nuclides. Because of my historical interest in the field I'll be the lead-off speaker there, unless they can get Dr. John Lawrence or Professor [Glenn] Seaborg, and I doubt that they will. I haven't talked to Dr. John yet. He's my intellectual godfather. He's the man who "turned me on," with his Sigma Xi lecture at Ohio State in November 1940. Anyway, I came out and worked with Hal on the I-123 in 1961.

The I-125, again because of its convenient 60-day half-life, took off immediately in the early '60s. By 1964 I was invited to go to Wiesbaden, Germany, to talk about iodine-125.

There were sceptics of that, too. A lot of people said, "It won't fly." All that sort of stuff. But, by and large, other people didn't pay any attention to them.

Hughes: What were their reasons? Did they have any?

Myers: The radiation wasn't hard enough. Lots of it was sour grapes; they hadn't thought of it first! But this came out of my student's M.Sc. thesis—Johannes C. Vanderleeden—in biophysics in 1959, which gave us priority. Being a practical sort of soul, I would send this fellow, who later

took his Ph.D. degree at Ohio State in physics, to the grocery store to get some sliced longhorn cheese. If you put a micrometer on it, you'll find that these slices are very uniform. So instead of using aluminum or copper or tin absorbers, we used longhorn cheese! We are much more like cheese, chemically, than we are like aluminum and so on. [laughter] The physicists were laughing at us when we were talking about cheese half-thicknesses, and so on and so forth. But this was a very good medium. It worked out fine.

I remember in 1960 I got a letter from Rosalyn Yalow. I don't know whether you know her or not, or know of her.

Hughes: I know of her.

Myers: I told her, "Quit fiddling around with that stupid iodine-131 and switch over to I-125." Well, she laid her ears back; she's very stubborn. But she always left an opening like, "It should be looked into further, but right now I'm too busy to look into it." Something like that. We've come to know each other very well over the last 33 years. She's coming to Ohio State next Friday to give a talk at the Ohio Academy of Science, and I'll see my old friend again then.

Anyway, every time I'd see her I'd say, "Now look, Ros. Quit fiddling with that I-131 and switch to I-125, and your method will take off like a jet." Finally, about 1967, she switched. She acknowledges now that, had she listened to me years earlier, her methodology that she worked out with Saul Berson would have become a world-wide thing much earlier.

Hughes: That's interesting. When you talk about clinical applications of all these various forms, is it mainly the radiologists that are taking it up?

THE SOCIETY OF NUCLEAR MEDICINE

Myers: Well, actually, some radiologists tried to control nuclear medicine as it was emerging. They tried very hard to do that. I don't want to take your time on the politics of it, but they really tried to do it through the AEC. The Society of Nuclear Medicine owes much of its origin because many other radiologists were incensed with the autocratic efforts on the part of one or two people to grab these new opportunities; but more particularly, many other people who were not radiologists. So the first meeting, held in Spokane in 1954, was merely a... Well, the Society had its origin, really, from that basis.

Hughes: So the formation of the Society was more or less to counter the efforts of [these few self-seeking] radiologists?

Myers: Yes, that's right, to a large extent.

Hughes: Was it rough sledding for a while?

Myers: Yes, very rough sledding. I'm on the executive committee of the Society; we're meeting in Sausalito on Monday and Tuesday. That's the reason I'm here. They asked me to bring along some of this early history.

In 1951 a radiologist from Philadelphia succeeded in getting through the AMA a resolution that radium, its disintegration products, and radioisotopes should be under the control of [those] certified by the American Board of Radiology, period. This incensed me, naturally, because I knew [Georg] Hevesy fairly well. He was a chemist, and so am I basically a chemist. I didn't want to see all these wonderful new opportunities grabbed onto by people that had no idea of the opportunities of applications

of radioisotopes in biochemistry and physiology.

In 1955 I put together a resolution in the AMA to rescind this previous action. Well, getting the House of Delegates of the AMA to eat its own words is not an easy thing, and I didn't succeed. But in 1956 I went through the Ohio State Medical Association and had it introduced again. There was one hell of a fight within the AMA at that time, in Chicago. But finally the speaker, Askey, from Los Angeles, said, "These people don't know what they're fighting about." I was in the back room when this issue came up. He said, "Is the author of this resolution here by any chance?"

I said, "Yes." I held up my hand.

"Would you please come up and explain to us what this is all about?"

So, I went up and said I felt that, as a physician, anyone should be able to use anything that is useful in the practice of medicine and not be limited by private corporations called specialty boards. I said, "This is a dangerous precedent that the AMA has accepted. This will, in the long run, depreciate the significance of the M.D. degree, and therefore it will be a disservice, at least." One of the trustees came up to me afterwards and said, "You're so right!"

Anyway, I said, "By analogy, since aspirin—which is the most commonly used drug in medicine— is usually taken internally, obviously aspirin should only be prescribed by one who is certified by the American Board of Internal Medicine." That clinched it! Again, there's a long story with that, but finally in the December 1956 meeting they rescinded the previous damn fool resolution of 1951.

I have the correspondence upstairs from Robert Newell no less, from San Francisco. He was a radiologist. He didn't want his colleagues in radiology to be able to

control this 100 percent. He had been a chancellor of the American College of Radiology, no less.

The issue was whether or not, if this stayed on the books, it would be of any significance to try to form a national society of nuclear medicine. At the third Annual Meeting I was in Chicago, fighting. He and I had corresponded on this. He was at the meeting in Salt Lake City. His letter, which I have upstairs, said that they stood around until the wee small hours of the morning about this issue, before standing room only.

When organized medicine rescinded its previous damn fool thing, that made it wide open, and then the Society of Nuclear Medicine merged into a national society. That was the beginning of that.

Hughes: I'd like to hear a little bit more about the Anger camera and all that instrument development, because my reading between the lines is that it took quite a while before Anger's prototype was actually being used actively in everyday medicine. Well, not everyday medicine, but on a wide scale.

THE FIRST COMMERCIAL ANGER CAMERA

Myers: You're talking to the man who had the first commercial version of Anger's camera at Ohio State University in September 1962.

Hughes: Yes, I realize that. But there were problems, weren't there?

Myers: Well, like the first of anything, there were a lot of problems with it. In the two volumes of [the] meeting of the International Atomic Energy Agency held in Athens,

Greece, in April 1964, mine was the only paper on a scintillation camera.

Anyway, there were real sceptics regarding Hal Anger's camera. I remember that the first scintillation camera was delivered to me in September of 1962, not quite twenty years ago. Immediately the company that built it, Nuclear-Chicago, wanted to borrow it back to display it at the radiological convention in Chicago. They wanted me to go along as a guest of some sort.

I loaned it to them, through our proper channels and all that. There was a fellow from Picker that had one of these to-and-fro "atomic flame throwers," as I call them. He wanted to know what the camera resolution was. Well, I had already made some pictures of a mercury-203 compound in rats' kidneys. Well, people know how big rat kidneys are, and they know how far apart they are. It was very sharp. No, no, he had to have it in millimeters, and so on.

During the meeting I suppose there weren't more than, *at most*, a dozen people [who] ever went to see this first commercial version of Hal Anger's camera. That was in December of '62, and mine was the only paper at a truly international meeting in Athens in 1964. In '65 I gave seven lectures at a meeting of the American College of Physicians at Johns Hopkins. I talked about the scintillation camera in one, and I might as well have talked to the birds about it. There was no interest whatsoever.

Hughes: Why? Do you have any idea?

Myers: Well, they were used to something that went "bang bang" and "clack clack," and the lights lit, and so on and so forth—"atomic flame throwers." It's a case of it's hard to teach old dogs new tricks. But I saw the potential of it right away. It's a long story of how we happened to get the money to buy that first one, but that's another story.

Hughes: Was it just a matter of time, then, for it to eventually catch on?

Myers: Well, let me think. Before going to that meeting in Athens in April 1964, it had become bruited around, throughout the world, that I had this instrument, and I was invited to lecture, on the way to Athens, in Dublin, London, Wiesbaden, and two places in Paris.

At the meeting in Athens the fellows that were running it for the IAEA, they were there just suppressing yawns most of the time and trying to be polite, and all that. But when I got up to speak—not because it was I, but because of Hal's instrument—they came and sat down in the front row so they could see the whole thing. That helped, certainly.

Then in '64, later—this was in April—in June of '64 we had our annual meeting of the Society of Nuclear Medicine, here at the Claremont Hotel in Berkeley. By that time, Nuclear-Chicago finally had wakened up to the fact that they had something very significant on their hands. They demonstrated here, in Berkeley, the 11.5-inch scintillation camera. It began to take off, really, at just about that time.

The other thing that made it take off was the fact that the same year that Hal and I, in '62—twenty years ago—introduced I-123, that was the same meeting in Dallas in which technetium-99m was introduced into medicine. The two worked together beautifully. The technetium-99m was a physical [tool]. It's of no use chemically, of course, [or] biologically. It's a physical thing that matches the Anger camera. And you could get it from a relatively long-lived cow and get it conveniently, and all that.

I remember I got a letter from Hal early in 1964. I remember he sent to me some pictures they'd made here with the camera at that time, that I could take along and

show in Athens. At the same time he wrote about a new isotope that was just perfect—technetium-99m. If it hadn't been for the Anger camera, technetium-99m would be of relatively little interest today, and vice versa. So, it was a natural wedding that made the whole thing fly.

Hughes: And Anger was the one that recognized the—?

Myers: Well, no. I don't know who it was. This was two years after it was introduced. There were improvements, too, in the camera, improvements particularly in the phototubes. They call them photomultiplier tubes. They're not. They're multiplier phototubes. It doesn't multiply the photons at all; it multiplies the electrons. The Germans call it ***Elektronenvervielfacher***, electron multiplier. That's what it is. It doesn't multiply photons. The photons are multiplied in the crystal.

Anyway, when Hal and I were working here together in '61, we had trouble with the 159-kilovolt gamma ray because the phototubes were not sufficiently discriminative. Early in '64, RCA came out with greatly improved bialkali cathode phototubes, and this made it possible then to go down to lower energies. There were improvements all the way along the line to make the thing fly and take off.

I have a picture of Hal Anger that I made in Heidelberg in 1966, a Kodachrome of him there. We were invited, each of us, to come over. Actually, I was invited to come over and talk about medical cyclotrons, which was the nearest, dearest thing to my heart, at the Gesellschaft für Nuklear Medizin meeting in Heidelberg. Hal was there. By this time it was fairly well known, and people were clawing at each other, trying to get one. The company was raising the price, the price, the price. Our first one cost us less than $17,000. Now with attached computer it's ten times that, at least.

Hughes: Let me backtrack a little. I told you that I was going through some of Hal Anger's correspondence which he gave to Lawrence Berkeley Laboratory.

Myers: He asked me to seal our early correspondence. I made a collection of all the correspondence he and I had on the scintillation camera. It's about that much [gestures] correspondence, back in the late '50s and early '60s. He told me to seal it and ensure that it would not be opened until after his death.

GEORG HEVESY

Myers: I wrote to Professor Hevesy back in '64. He died the following year. I suggested that the next time he came to the States he should come and visit me. He had promised years previously that he would come and lecture to my students, but he was dying of cancer of the lung in 1964. I suggested that he should bring Hal's discovery to the attention of the Nobel Prize Committee for Medicine. But it's too late now. Nevertheless, Hal Anger's scintillation camera has turned medicine "inside out," literally, in less than twenty years!!

I gave the Blumgart Memorial Lecture in Boston in October [1981] and reviewed the early work. The man who started nuclear medicine was Hevesy, in 1913, with Fritz Paneth. They wrote a paper, "Ueber Radioelemente als Indikatoren in der analytischen Chemie." I have that paper at home. If you look at the two volumes by Hevesy, entitled *Adventures in Radioisotope Research*, there are a hundred papers there, and these are the English translations of those in other languages. It's all there, with historical interpolations among the papers. That's very important, *very* important. Hevesy was a chemist. His Ph.D. was in physical chemistry, as mine was. He got it in

1908 in Freiburg, Germany.

The man who first used [a radioactive tracer in medicine] to my knowledge—I always have to say that because sooner or later somebody will come along and send me a reprint showing how wrong I was—was Hermann Ludwig Blumgart in Boston, beginning in 1924. Hevesy carried out his first research experiment with Paneth in the field of inorganic chemistry. They determined the slight solubility of lead sulfide and lead chromate in water, with lead-210. In 1923 he carried out the first experiment in biology, with bean seedlings, with lead-212 with its 10.6-hour half-life.

The following year he carried out studies with I. A. Christiansen and S. Lomholt, published in *Comptes rendus*. Hevesy had sent me 75 or 80 reprints of his 400 papers; I have them at home, along with many of his handwritten letters that he wrote, even after he had the Nobel Prize. He had no secretary. He used a clock on the wall. He made his own gold-leaf electroscope. That was the way Hevesy worked.

I've heard him lecture in English, of course, French, German, Danish. I'm sure he could lecture in his native tongue, Hungarian. It never seemed to make any difference to him. Whatever the language that was required, that was it; he just did it!

NEUTRON THERAPY

Hughes: Were you ever interested in neutrons as a form of cancer therapy?

Myers: Well, I am now. And, of course, Aebersold, his doctoral dissertation—you should read that, or at least be acquainted with it—it was entitled *The Collination of Fast Neutrons*. That was the device that was used by Robert

Stone in the first uses of neutrons in 1937 and '38, with the old 37-inch cyclotron, for treating cancer. So that was the beginning of it.

I knew Stone. He told me he was very sorry he had ever turned neutrons loose on anyone. They really burned the hide off these people. They lived longer, but they'd have been better off dead.

Hughes: Was that just lack of knowledge about the biological effects?

Myers: [inaudible] The first paper on the biological effect of neutrons was by John and Ernest Lawrence, in 193–. Let's see. The neutron was discovered in '32, in February. Then there was a paper by one of the Lawrences, Aebersold, and the other Lawrence, in '36 I think it was.

Hughes: John Lawrence begins his visits to Berkeley in 1935, and that's when the biological experiments start. [Discussion of papers omitted.]

Myers: Seaborg—I've heard him tell it many times—that he and [J. J.] Livingood discovered iodine-131 on the basis of a conversation he had with Joe Hamilton in LeConte Hall. Hamilton was using iodine-128 at that time, with a 25-minute half-life. Seaborg said, "How are you doing, Joe?"

He said, "Fine, except that the 25-minute half-life is too short. We can't study the physiology very well."

[Glenn] said, "How long do you want it?"

[Joe] said, "Oh, about a week."

So, they cranked up the old 37-inch cyclotron and discovered I-131, with an 8-day half-life. That's what I call "custom discovering." [More discussion of papers omitted.]

This was a picture that I made of Joe Hamilton in April 1956, less than a year before he died of leukemia. I got to know Joe best at Bikini. Between the two explosions out there, we used to study statistics with two little white cubes with six black spots on them! Then he and I had our exhibits beside each other in London in 1950. He was in charge of the 60-inch cyclotron up here on the hill. And I said, "Joe, do you mind if I take your picture? It's very colorful."

"No, go ahead."

I was surprised when I saw that he died of leukemia in February 1957.

Hughes: Was that radiation-related, do you think?

Myers: Well, Low-Beer died of leukemia; Irène Curie died of leukemia; her mother died of leukemia.... [Discussion of papers omitted.]

THE SOCIETY OF NUCLEAR MEDICINE EMBLEM

Myers: This [the emblem of the Society of Nuclear Medicine] might be of some interest. There's quite a little bit of history in that. This was concocted as a result of a cocktail party at Dr. John Lawrence's house in 1972. He had been the president of the Society of Nuclear Medicine. He had the plaque on the wall in his home, [for] his presidency of the society. I said, "John, you must be very pleased with this." He said, yes, that was a very eventful year. That was the same year his lovely wife Amy died. But he carried on.

I said, "There's only one thing wrong with the plaque." At that time, if you look up the old symbol of the Society, it had four electrons rattling around the snake, encircling it.

[John] said, "What's that?"

I said, "Well, this represents beryllium, a very noxious element."

He said, "Only a chemist like Bill Myers could think of something like that."

So the next day I told Ellis Myers in graphic arts about it and he said, "How many electrons should it have?"

I said, "Well, the radioisotope with the minimum number of electrons that can be 'seen' from inside out is carbon-11. Moreover, one shouldn't violate the sacred symbol of medicine. It should *rest* on the sacred symbol of medicine."

So he came back and said, "Putting in these six electrons makes it look messy."

I said, "Well, use the Bohr model [of the atom] and put the two K electrons and then here are your four." So, that's now the symbol of the Society of Nuclear Medicine.

Hughes: Does the Society of Nuclear Medicine have any artifacts of any kind?

Myers: No. With the preciousness of space, especially in New York City, we can't. Well, we have an archives, but that is just a file cabinet. It's the same way at the American Institute of Physics. Their archives is a file cabinet. I saw it when I was there. I gave them many of these pictures. Because of the preciousness of [them], they were going to make film copies and store those in a vault.

GEORG HEVESY (continued)

Myers: Hevesy had one son and three daughters. We wanted the middle daughter, Ingrid, to bring her mother, Pia, to come and hear the first Hevesy lecture of the Society [of

Nuclear Medicine]. Her mother was a severe diabetic so we in the Society supported the bringing of the son, Georg, who is a general practitioner from the outskirts of Stockholm, and Ingrid. They came to Atlanta.

You're aware, I'm sure, of these four or five big long films about Hevesy here on this campus that Hardin Jones had made in 1962. Well, I showed an excerpt from one of those, and Ingrid cried to see her father in action again. He died of lung cancer in 1965.

His son told me that he and some other physicians feel that his father's lung cancer may have stemmed from his use of radon in Niels Bohr's radon source, or possibly from the beryllium. They used to file beryllium to make a powder, and then they'd mix the radon gas with that. That was their neutron source. [Discussion of papers omitted.]

[Hevesy] and Niels Bohr were very, very close friends, and he wrote many of his papers from the Institute of Theoretical Physics. His early biological papers even came from there.

Your best possible single source of the early history of nuclear medicine was a symposium held in San Francisco by the Radiological Society of North America in December 1941.

Hughes: I've seen that, on the cyclotron.

Myers: I handed this out to my students for over twenty years. That gives the early history of the cyclotron, beginning with the one up on the hill, the little 4-inch one. That symposium was published. It occupied the whole November 1942 issue of *Radiology*.

THE ORIGIN OF THE TERM "NUCLEAR MEDICINE"

Hughes: Let me ask you one more question. What about the term "nuclear medicine" itself? Do you remember when that [was coined]?

Myers: There was a question whether it was going to be called nuclear medicine or atomic medicine.

Hughes: Well, there's that book by [Charles F.] Behrens, called *Atomic Medicine*.

Myers: Yes, and the first three volumes of Dr. John Lawrence's *Progress in Atomic Medicine*.

Hughes: Right.

Myers: He changed it to... [*Progress*] *in Nuclear Medicine* just two issues back.

The term nuclear medicine originated right here in San Francisco. Professor Robert Newell is said to have coined it.

Hughes: Well, of course, the Society [of Nuclear Medicine] was formed in 1954, wasn't it?

Myers: Yes, 30 May 1954.

Hughes: Obviously they were using the term then. I just wondered how much before—?

Myers: The first time I've ever seen it in print was, I think, in '52 in the *American Journal of Roentgenology, Radium Therapy and Nuclear Medicine*, the January issue. That

was the first time.

The argument was whether it should be called atomic medicine or nuclear medicine. Newell was a radiologist and said it should be called nuclear because the whole thing depends upon the potential energy pent up in the unstable nucleus. So, he won out, and rightly so.

For a long time I would have chosen atomic medicine because we are using these in the Hevesy sense, as radioindicators, special chemical reagents, and therefore I would have called it atomic medicine. But I think it was a *comme ci, comme ça* thing, which way you wanted to go.

Hughes: What did you think of yourself as, in the early days?

Myers: Oh, adventurers, I suppose. I don't know.

Hughes: Is your medical specialty hematology?

Myers: No, I don't have any medical specialty. I specialize in whatever interests me at the moment. I refuse to be nailed down. My wife is the same way. She did general practice for 33 years. She specialized in the skin and its contents.

TERMINOLOGY

Myers: Dr. John [Lawrence] was the father of radiopharmaceuticals, although he hates that term as much as I do. But there *is* a place for it; that is, where you're deliberately using it to produce a pharmaceutical effect—a biological effect—you call it a radiopharmaceutical. Otherwise we should use Hevesy's term, radioindicators, which now are being called "tracers."

Hughes: And radioisotope is the umbrella term?

Myers: No. Radioisotope is not a proper term, really. In 1947 T.P. Kohman introduced the term radionuclide. "Iso" is a Greek word for same, and "topo," place. Same place. They're the same place in respect to atomic number, the Z. But this doesn't tell you the fact that, in the case of iodine, one has 29 different species with the same atomic number of 53. So, Kohman said we should call them radionuclides, so that we will have a specific designation for not only the Z, and the A in particular, but also the energy content. This is the term that I use most of the time. I only use the term isotopes in speaking to lay persons. In my own terminology I use the term radionuclide. Iodine is a good example: 29 different radioisotopes and they all have the same Z. But there are 29 different radionuclides. [Discussion of photograph omitted.]

P-32 was discovered in Rome in 1934 by Enrico Fermi and his students, including Emilio Segrè. The first use of P-32 was by Hevesy in 1935. That was as a tracer. He discovered the dynamic state of the body constituents with P-32, which he made. He took 10 liters of carbon disulfide and dunked the radium-beryllium source of Niels Bohr in it. He put this down in a deep hole in the laboratory floor so that it wouldn't bother people, and he left it there for a couple of weeks. Well, the neutrons in the (n,p) reaction on sulfur-32 gave the P-32, and all he had to do was to burn off the carbon disulfide, which burns readily, and he had the phosphate. He never had more than a microcurie, though.

Do you know Hilde Levi by any chance?

Hughes: No.

Myers: Well, she is Hevesy's chief biographer. She's just retired. Georg Hevesy's son wrote me last year that she's putting

together a book on Hevesy. She and Hevesy discovered neutron activation analysis in 1936. She's a very charming individual. I'm sure there's lots of correspondence in Donner Lab. Jan DeMoor would have it. [Discussion of photographs omitted.]

HEVESY NUCLEAR MEDICINE PIONEER LECTURE

William G. Myers

No one is more dedicated to nuclear medicine than William Graydon Myers, Ph.D., M.D., D.Sc. (honorary), and FACP. As he has often said, he has had a life full of "twinkling" atoms and "scintillating" people. The latest edition of Who's Who in America describes him as "physician, scientist, educator." He is all of these, but much more. The younger members of our Society will remember him best as our Historian, a position he has held since 1973. Among the aphorisms that he cherishes are: "Savoring the past enriches the present and presages the future"; and "History cements facts together." Lest we think he is being pretentious with these offerings, he adds: "Laughter lubricates life."

The history of nuclear medicine is reflected in the life of its historian. His pioneer spirit was evident early in his life—in 1928, he played the lead role in a play at the Ridgeville High School in his home town of Toledo, Ohio. The play was "The Go Getter." The role was that of Don Estaban Pompo, Alcade of Pagola, a small village in the principality of Casadonia, a fictitious country somewhere in southern Europe. We do not know why he never pursued the life of an actor after such great initial promise. We next encounter young Bill in 1932 when he wins the 125-pound intramural boxing championship in the Ohio State University Coliseum.

It is interesting to read the referee's instructions given the fighters in those bouts:

> In the case of a knock-down in which the contestant is in such a dazed or helpless condition that he would be unable to defend himself against a complete knockout, the round ends. After two minutes rest, the next round will be started.

Those of you who believe that Bill's life has always been a bed of roses should know that, from time to time, he has had to rebound after 2 minutes' rest. His early life included a newspaper route at age 5, a period of time in an orphanage, homesteading on the Canadian prairie, a job as a photographer in Denver, a waiter in Detroit, and a barber while attending college. He barbered his way through a Bachelor's degree in 1933, a Master's in 1937, a Ph.D. in 1939, and an M.D. in 1941, all from Ohio State. It wasn't that he liked Ohio State so much. It was just that he liked the barbershop in the old Starling Loving Hospital.

As a senior medical student in 1940, Bill began the fulfillment of his destiny. If Ernest Lawrence is the father of the cyclotron, Bill Myers is its godfather. Artificial radioactivity had been discovered in Paris only 6 years before when Ernest Lawrence's brother, John, gave a Sigma Xi lecture attended by Bill at Ohio State. When he heard John Lawrence's inspiring description of the early experiments with the first manmade "twinkling" atoms created in the Berkeley cyclotron, Bill decided then and there that nuclear medicine would be his special

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field of interest and endeavor. Together with his precious atoms, he himself began to "twinkle" and has done so continuously for 40 years.

His senior medical student thesis was "Applications of the Cyclotron and Its Products in Biomedicine." It was accepted in February 1941 by Professor Charles A. Doan, the distinguished Chairman of the Department of Medicine who was to prove subsequently to have an important, though little known, role in the development of our specialty.

The same year, 1941, Ohio State University got its own cyclotron, a 42-inch machine for use in both physics and medicine. While still a senior in medical school, and during his internship at Ohio State University Hospital, Bill generated phosphorus-32 to treat patients with polycythemia rubra vera, and sodium-24, potassium-42, and strontium-89 for physiological studies. From 1942–45 Bill was a Research Associate at the Ohio State University Research Foundation. In 1946 he traveled to the Pacific to serve as a Radiological Safety Officer for the atomic bomb tests at Bikini Atoll. By then he was a close friend of John Lawrence, and he recalls:

> We were sitting, geiger counters in hand, on the deck of a little gunboat when the bomb was detonated. It was awesome, more than a million tons of water were thrown a mile into the air. Battleships anchored near the blast site sank instantly. The Arkansas, all 25,000 tons of her, stood on end, and was tossed around like a toy.

He also remembers dreaming aloud at that time with other physicians working on the project. They shared ideas about what could be brought back home and put to use in laboratories and hospitals.

He began then and has continued his devotion to the chart of the nuclides. "I would look at the chart and imagine that this nuclide and this and this might be useful in medicine. I would say to my colleagues 'Here's one we ought to take a good look at.' " More often than not, his good looks paid off and a new nuclide was added to medicine.

A striking example can be seen in his laboratory notebook on August 6, 1945, the day the atomic bomb was dropped on Hiroshima. He describes bombardment of boric oxide with deuterons to produce carbon-11 monoxide. Only today, 36 years later, has carbon-11 achieved a preeminent place in nuclear medicine.

Just as nuclear medicine started with the cyclotron and was diverted by the nuclear reactor, only to return to the cyclotron as a major source of radionuclides, so also did Bill. During talks with people aboard ship at Bikini, from people such as his dear friend Paul Aebersold, he learned that cobalt-60 and gold-198 could be produced in tremendous quantities with the second reactor in the world, the graphite reactor at Oak Ridge National Laboratory. He immediately saw the advantages over radium-226 and radon-222 for cancer treatment.

In June 1948 he reported to the American Radium Society his initial findings with cobalt-60 and correctly predicted that thousands of cobalt-60 units would someday be installed in cancer treatment centers throughout the world.

In June 1952, again to the American Radium Society, he reported the use of gold-198 for permanent implantation into cancers, a second form of therapy that displaced radon-222 "seeds" in radiation therapy.

By 1954, about the time 12 persons were in a Spokane hotel agreeing to put up $10.00 each to start an atomic medical society that became the Society of Nuclear Medicine, Bill moved into a spacious new laboratory at Ohio State University, returned to his true love, the cyclotron, and began producing carbon-11 and fluorine-18, the nuclides of the 80's in nuclear medicine. First, he learned how to make carbon-11 glucose in a crude form by "feeding" $^{11}CO_2$ to bean leaves and algae.

Two other nuclides were developed by Bill—chromium-51 in 1958 and iodine-125, 2 years later. Reporting the seventh annual meeting of the Society of Nuclear Medicine in Estes Park, Colorado, *Time* magazine in its July 4, 1960 issue included an article on "Atoms and Man," part of which said:

> Last week Ohio State University's Dr. William G. Myers reported that a seventh isotope of iodine, iodine-125, shows promise as a convenient tracer to follow the metabolic pathways of ordinary iodine. But it has not yet been used in humans.

Today iodine-125 is the most widely used radionuclide in medicine, bearing out Bill's prediction of its value in the late 1950s. Hundreds of millions of iodine-125 tests are performed worldwide each year. He cherishes a handwritten note from Nobel laureate Rosalyn Yalow:

> With cordial greeting to Bill Myers—a fine friend for more than three decades—with fond memories of his suggestion in 1960 of the applicability of ^{125}I rather than ^{131}I for tracer labeling—a prediction that proved most valuable.

A second isotope of iodine, iodine-123, was strongly advocated by Bill in 1962. If it could be made available from Los Alamos or Brookhaven National Laboratory in large quantities, it could join carbon-11 and fluorine-18 as a major nuclide of the 80's.

In addition to his role in developing new radionuclides, Bill played a major, although little recognized, role in the development of the scintillation camera. Again it was the case of a prepared mind. Four years after Benedict Cassen had built the first rectilinear scanner, Mueller and Myers described an improved version—called the "Gammicon"—designed and built in the Department of Electrical Engineering of Ohio State University. Their report concluded: "New tools invariably provide new

opportunities to accelerate progress." George Mueller, then Associate Professor of Electrical Engineering, later became Director of NASA's Manned Space Flight Program and was in charge of putting Neil Armstrong on the moon.

The story of how the Gammicon was invented illustrates how Bill has been able to inspire people all his life. Over lunch at the faculty club, Bill described to George Mueller what was needed to carry on his work with the "peaceful atom." They discussed the specifications. A year later, he invited Bill to visit his laboratory with the comment: "Why don't you stop by one of these days to see something that might interest you?" Dr. Myers walked into Dr. Mueller's office in the Caldwell Electrical Engineering Laboratory and there was the Gammicon—the machine the two had talked about a year before. To produce the scans, Mueller had used the Teledeltos system developed by Western Union to transmit pictures.

Meanwhile, Hal Anger, embodying in a single person a convergence of nuclear physics, electronics, optics, and information handling, was pursuing a completely different path from the rectilinear scanner. Working in the Donner Laboratory of Medical Physics of the Lawrence Berkeley Laboratory, he used a simple, pinhole collimator in front of a sodium iodide crystal 4 inches in diameter. Behind the crystal was an array of seven photomultiplier tubes. The output of the tubes was used in a way such that the location of each scintillation could be identified by grid coordinates and flashed on a cathode ray tube in the same relative position, which could then be photographed. The key was that the new camera provided a continuous measurement of the time course of the radioactivity in all regions simultaneously.

In June 1958 Anger first exhibited the scintillation camera at the Society of Nuclear Medicine meeting in Los Angeles and also at the AMA convention in San Francisco the next week. The potential value of the camera was recognized immediately by Dr. Myers, but by few others. In his characteristic way, he spent the next year trying to interest several manufacturers of nuclear medicine instruments to begin production of the Anger camera. He was unsuccessful. Subsequently, three of the companies that he approached became major manufacturers of scintillation cameras.

In 1960 with $17,500 supplied by Dr. Charles Doan, Chairman of Medicine at Ohio State University, Bill placed a special order for the first commercial Anger camera with the Nuclear Chicago Company. He had to threaten to sue to get them to finish and deliver it, but, indeed, in September 1962 the first industrially-fabricated scintillation camera was installed in his laboratory at the Ohio State University Hospital.

In 1964 at the International Atomic Energy Agency symposium on "Medical Radioisotope Scanning," Dr. Myers presented the only Anger camera paper on the program. In the publication that followed, he published the first images made with a commercial Anger camera, a series of images of a rat injected with [^{131}I] orthoiodohippurate. He concluded his paper: "The scintillation camera is shown to provide an elegant method for the study of dynamic processes in vivo that are not otherwise demonstrable."

Even in 1968, 10 years after the first presentation of the Anger camera, when David Kuhl and I presented a series of 48 lectures in Australia, the principal topic was the relative merits of cameras versus scanners. It takes a finite—sometimes surprisingly long—period of time for inertia to become momentum. People such as Bill Myers provide the direction and energy. Professor Kurt Scheer of Heidelberg, Germany, has stated: "It has been my observation that when Bill Myers sits and thinks, the whole world of nuclear medicine benefits."

Nuclear medicine has made it possible for Bill to develop bonds of mutual friendship and respect with people all over the world. When Professor Scheer was planning his new Institute for Nuclear Medicine in Heidelberg in 1962, Dr. Myers suggested that he should plan to install a cyclotron. He did and the cyclotron has now been producing neutron-deficient radionuclides for nearly a decade.

The Chinese philosopher, Lao-tzu said: "Leaders are best when people scarcely know they exist. When their work is done, their aim is fulfilled, the people will all say: 'We did this ourselves.' "

A final example. In November 1964 Bill received a phone call from a physicist in Berkeley—Alan Fleischer—who told him that he and others were thinking about establishing a company to build cyclotrons, chiefly for medicine. Bill's ideas about medical cyclotrons played an important role in their decision to go ahead with the venture.

Shortly after Bill retired from Ohio State in 1979, after being on the faculty for 46 years, he was appointed Visiting Professor of Biophysics at Sloan-Kettering in New York. He spent most of the next 2 years working with the first medical cyclotron fabricated by this company in 1967, carrying out experiments with potassium-38, carbon-11, and nitrogen-13. After four decades, he had come full-circle back to the cyclotron.

It has been my great fortune to be a friend of Bill Myers. Some persons are old at 50; others, like Bill, are young at 73. His studies of himself with cyclotron-produced radionuclides have proved without doubt that the blood flow to his brain continues to supply that extraordinarily sensitive structure with an abundant supply of oxygen and sugar, permitting his creative mental processes and pleasant social relations to continue full tilt. Not to say that he doesn't at times speak too long extolling the virtues of brevity at meetings of the Executive Committee of the Society of Nuclear Medicine. He is probably the only person ever to have written a five-

page article pointing out how few words there are in Lincoln's Gettysburg address.

Bill has been blessed by a good and long life—accompanied by his beautiful, devoted, and accomplished wife, Florence, also a medical graduate of Ohio State University. She wrote a paper with Bill in 1945, describing the first patient treated with penicillin in Columbus, Ohio. Ever since these early days he has developed one idea after another in natural progression. He has managed to avoid the administrative duties that so often decrease productivity. He has remained clear-headed and open-minded, keeping up with his field by constant reading and attendance at scientific meetings. A major factor in his continued productivity over 46 years is association with students to provide intellectual stimulation. In addition to his teaching at Ohio State, for over 25 years he taught a course in nuclear physics, chemistry, and medicine at the University of California at Berkeley.

Throughout his life, he has communicated to his students the joy in his mind. Inspired by Bill's single-minded love of radioactive indicators, his students brought him willing hands, new points of view, and enthusiasm. He gave them his insight and experience.

His honors are too numerous to mention, except for one that he especially cherishes. In 1973 he received the first Paul C. Aebersold Award for Outstanding Achievement in Basic Science Applied to Nuclear Medicine. He was presented the award by Mrs. Aebersold, a friend of long standing.

Now he joins the most elite group in the nuclear medicine world. He has been officially named as a Nuclear Medicine Pioneer, joining many other great scientists whom he knew personally—Ernest and John Lawrence, Lise Meitner, Paul Aebersold, Irene Curie, and most of all, his beloved Professor George de Hevesy, whom Bill has called the Father of Nuclear Medicine.

Bill, your numerous contributions to nuclear medicine have won for you the respect of all who honor excellence. You have taught us what true freedom is; you have fulfilled your individuality.

You can know that there will be continuing benefits long after your efforts have ceased. You have had the pleasures of discovery, the opportunity to spend your life doing what you wanted to do, the freedom to study and investigate, to develop friendships all over the world, and to know that your teaching and research have helped all mankind. All these satisfactions are yours. No one could ask for more.

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INDEX

Aebersold, Paul C., 5, 6, 7, 8, 14, 29, 30

——, *The Collination of Fast Neutrons*, 29

Aebersold, Paul M., 5

American Board of Internal Medicine, 23

American Board of Radiology, 22

American Chemical Society Journal, 11

American College of Physicians, 25

American College of Radiology, 24

American Institute of Physics, 32

American Journal of Roentgenology, Radium Therapy and Nuclear Medicine, 34

American Medical Association (AMA), 19, 22–24

Anger, Hal O., 8, 17, 19, 20, 24–28

Armstrong, Neil, 18

Askey, , 23

Behrens, Charles F., *Atomic Medicine*, 34

Berson, Saul, 21

Bikini Atoll (bomb tests), 1–4, 5, 6, 31

Blumgart, Hermann Ludwig, 29

Blumgart Memorial Lecture (Boston), 28

Bohr, Niels, 32, 33, 36

Calvin, Melvin, 11

Cassen, Benedict, 18

Christiansen, I. A., 29

Comptes rendus, 29

Curie, Irène, 31

Curie, Marie, 31

DeMoor, Jan, 37

Doan, Charles Austin, 1, 3, 4

Donner Laboratory. *See* University of California, Berkeley

Durbin, Patricia, 9

Ehrlich, Paul, 12

Eniwetok, 14, 15

Fermi, Enrico, 36

Gesellschaft für Nuklear Medizin, 27

Hamilton, Joseph, 2, 3, 9, 30, 31

Harwell, 8

Hassid, W. Z., 11

Hevesy, Georg, 3, 22, 289, 32–33, 35, 36, 37

——, *Adventures in Radioisotope Research*, 28

Hevesy, Georg (son), 33, 36

Hevesy, Igrid, 32–33

Hevesy, Pia, 32–33

Hevesy Nuclear Medicine Pioneer Lecture, 38

Ile de France, 8

Institute of Theoretical Physics (Copenhagen), 33

International Atomic Energy Agency (IAEA), 24, 26

International Congress of Radiology, 8–9, 16

Johns Hopkins University, 25

Jones, Hardin, 2, 3, 33

Journal of Laboratory and Clinical Medicine, 4

Kamen, Martin, 11

Kobman, T. P., 36

Lawrence Berkeley Laboratory. *See* University of California, Berkeley
Lawrence, Amy Bowles, 31
Lawrence, Ernest, 1, 4, 11, 30
Lawrence, John, 1, 2, 3, 4, 17, 20, 30, 31–32, 34, 35
Levi, Hilde, 36–37
Liberté, 8
Lilienthal, David, 13–14
Livingood, John, 30
Lomholt, S., 29
Los Alamos National Laboratory, 5, 6
Lousche, Frank, 16
Low-Beer, Anne Treadwell, 9
Low-Beer, Bertram V. A., 9, 31

Manhattan Engineer District, 3
Memorial Sloan-Kettering Cancer Center, 10, 16
Mueller, George, 19
Myers, Ellis, 32
Myers, Mrs. William, 8, 8, 35

Newell, Robert, 23–24, 34, 34
Nobel Prize, 11, 28, 29
Nuclear-Chicago, 25, 26

Oak Ridge National Laboratory, 2, 4–6, 8
Ohio Academy of Science, 21
Ohio Journal of Science, 1
Ohio State Medical Association, 23
Ohio State University, 1, 3, 7, 10, 11, 13, 15, 20, 21, 24

Paneth, Friedrich, 28, 29
Pecher, Charles, 3
Perlman, Isadore, 20
Picker Corporation, 19, 25
Pool, Marion, 3
Progress in Atomic (later *Nuclear) Medicine*, 34

Radio Corporation of America (RCA), 27
Radiological Society of North America, 33
Radiology, 33
Ruben, Samuel, 11

Scott, Kenneth, 2
Seaborg, Glenn, 20, 30
Segrè, Emilio, 36
Sigma Xi, 1, 20
Society of Nuclear Medicine (SNM), 10, 17, 22–24, 26, 31–32, 34
Stone, Robert, 29–30

Truman, Harry S., 13–14

United States Atomic Energy Commission (AEC), 6, 13, 16, 22
University of California, Berkeley: Donner Laboratory, 3, 5, 17, 37; Lawrence Berkeley Laboratory, 28; LeConte Hall, 30
University of California, Los Angeles, 2

Vanderleeden, Johannes, 17, 20–21

Wagner, Henry M., 38
Warren, Stafford, 2–3
World Federation of Nuclear Medicine, 10

Yalow, Rosalyn, 13, 21

12 4325